Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

EDITORIAL

The suggestive article by Professor Schwarz relative to a former land connection between Africa and South America (p. 81) furnishes an excellent illustration of the vital dependence of our studies of most large problems on fundamental conceptions of the early states of the earth. An essential part of his argument from the nature of the rocks of the oceanic islands hangs upon theoretical views as to what the specific gravities and the structures of the oceanic and continental portions of the crust, respectively, should be, under alternative assumptions relative to the configuration of the ocean surface. If the average specific gravities of the sub-oceanic and of the sub-continental sectors were the same, the waters of the hydrosphere would be drawn up about the continental masses, and would add their gravity to that of the continents, and thereby increase the differential stresses within the lithosphere and tend to depress the continents. To sustain stresses of such magnitude, the rigidity of the lithosphere must be assumed to be very effective, and the agencies of elevation must have worked against these stresses ever since the continents were formed.

If, on the other hand, the sub-oceanic rocks are sufficiently higher in specific gravity to counterbalance (with the aid of the overlying oceanic waters) the weight of the continental protuberances, the surface of the ocean must be more nearly spheroidal and the lithosphere much less affected by differential stresses.

To test these alternative hypotheses in the most direct and positive way, there is need that geodetic measurements of the ocean surface be extended outward from the continents on chains of islands as far as possible, and that pendulum observations be made on the oceanic islands and on the open ocean itself, so far as practicable.

But even if such determinations were at command, there would still be need, whatever their results, to take careful note of the different inferences that legitimately arise from alternative views of the earth's genesis, if we are to proceed on safe grounds of interpretation. The criteria deduced by sound reasoning from a molten earth do not altogether hold if the earth was built up by gradual accretions and was solid at all stages.

If the earth was once molten, it is a fairly sure inference that all the deeper portions were affected by essentially the same specific gravities at the same distances from the center. Only at and near the surface is it probable that there was much differentiation of specific gravity in the original liquid spheroid, if indeed much even there. If, therefore, the geodetic determinations were to show that the oceanic surface is nearly spheroidal, and the pendulum observations were to show that the crust is in approximate isostatic equilibrium in a general way, neglecting local inequalities, it is a firm inference that there must be a rather marked difference in the specific gravities of the sub-oceanic and of the continental portions of the crust respectively, as Professor Schwarz has indicated, for the sub-oceanic crust, plus the water that is on it, must counterbalance the continental protrusions. The amount of this difference in specific gravity *must depend on the depth to which the differentiation of rock extends.* For example, if the continental protrusions be taken to average three miles in height above the oceanic bottoms, and the lower limit of differentiation of specific gravities be assumed to be reached at a depth of six miles below the average surface, the specific gravity of the differentiated portion of the sub-oceanic crust, three miles thick, must be enough, with the aid of the overlying water, to counterbalance the six miles of the differentiated continental shell, from which it is obvious that a very high specific gravity for the sub-oceanic rock is required. If the differentiated portion of the crust were no thicker under the continental surfaces than under the oceanic, the differences of specific gravity would be still greater. If the limit of differentiation be taken at the greater depth of nine miles below the average surface, six miles of rock and three miles of water on the oceanic side must counterbalance nine miles of rock on the continental side, in which case a less, but still a pronounced, difference in specific gravity is required to meet the conditions of the case. For any such moderate depth of differentiation as would probably arise in the development of a crust upon a molten sphere, a marked difference between the specific gravities of the sub-oceanic and of the continental rocks seems to be implied, and this furnishes a working basis for such inferences as are discussed in Professor Schwarz's article. These are representative of the lines of interpretation that have most prevailed under the dominance of the doctrine of a primitive molten condition.

If, however, the earth be supposed to have been built up by the accretion of planetesimals, with a concurrent differentiation into continental and oceanic segments through the agency of weathering and transportation, as recently suggested,[1] the differentiation of specific gravities between the sub-oceanic and the sub-continental segments may extend to a possible depth of 1,500 to 1,800 miles. With such a depth of differentiation, a difference of surface protrusion of three miles only requires an average differentiation of specific gravity of about one-fifth of one per cent., a difference quite beyond detection lithologically. If the depth of effective differentiation were much less, as is not improbable, the average specific gravity of the sub-oceanic rocks would still need to be only slightly greater than that of the sub-continental ones to meet the requirements of the case.

When we consider the wide range of variation that was likely to be introduced by selective fusion and by the magmatic differentiation of the extruded rocks of both the sub-oceanic and the sub-continental segments, it does not seem safe to infer that any specific class of rocks would be excluded from either area. The probable differences between the rocks of the two areas would be detectable merely by a greater preponderance of heavy rocks in the sub-oceanic areas and of lighter ones in the continental. If a compilation of available data, made some years ago at the suggestion of the writer, is to be trusted, the preponderance of basic rocks in the extrusions of the oceanic volcanoes, present and past, is quite as high as the planetesimal hypothesis requires, and the discovery of a notable percentage of granitic and other acidic rocks is quite consistent with this hypothesis.

Under the planetesimal hypothesis the segments now beneath the ocean were, at an early stage, parts of the land surface alike with the continental segments. They are presumed to have been gradually converted into ocean bottoms by surface differentiation, leading on to greater depression, the volume of the hydrosphere meanwhile increasing and assisting in the submersion. The portions now submerged were presumably affected by reliefs not unlike those of the continental portions at like stages of evolution. In the course of their gradual submersion, the more protuberant swells and ridges are presumed to have stood forth from the growing seas as variously shaped lands which doubtless had a dominant tendency to elongated swells of ridge-like aspect, such as now affect the ocean bottoms and are being brought out more and more as soundings multiply. Not that all are necessarily of this class, however. In the slow process of their submergence, these swells and ridges were doubtless subject to denudation and circum-decomposition, as are other lands, and hence are similarly attended by sedimentary rocks. Some of these may have been submerged only in the later deformations of the earth's body, those of the Tertiary period perhaps, and previous to this they may have constituted bridges between the continents, and thus have satisfied the requirements of biological data, if these are indeed requirements. The loss, through deformation, of such bridge-connections of comparatively limited area and of moderate depression makes a relatively small demand on dynamic agencies and involves the withdrawal of a relatively small volume of water from the continental platforms. On the other hand, if vast continents be supposed to have arisen from the depths of the Atlantic, Pacific, and Indian Oceans, and to have again subsided, not only is a heavy tax laid on dynamic resources, but great volumes of displaced water must be accounted for in a complete hypothesis. We can no longer leave these considerations out of account as in the past, for the days of legitimate appeal to *terrae incognitae* are over.

---

[1] Chamberlain and Salisbury, *Geology*, Vol. II, pp. 106–11.

If the hypothesis of a relatively thin shell shearing over a solid substratum, which seems to be forced upon us by a study of the folds of the corrugated mountains, be entertained,[1] the occurrence of foliated rocks at shallow depths beneath the oceans, as well as the continents, is to be assumed, though perhaps their extent and degree of development may be inferior. These foliated rocks may have as wide a range of lithological characters as the igneous rocks of the sub-oceanic crust which, as we have seen before, under the planetesimal hypothesis may have a range little short of that of the continents. The existence of various gneisses and schists is not, therefore, in itself discriminative.

---

[1] Chamberlin and Salisbury, *loc. cit.*, pp. 125–32.

It is not improbable that, in time, the notable chain of islands that now skirt the eastern border of the Asiatic continent from Sumatra to Kamtchatka, standing along the outer border of the true continental platform, will be degraded and a portion of their material carried toward the mainland and re-deposited, and that this, conjoined with continental detritus, will at length fill up the intervening seas with stratified rocks, and thus extend the mainland eastward to the vicinity of these islands. In the meantime, sea encroachment, downward flexure, and the other agencies that affect the borders of continents[1] may have brought about the submersion of the sites of the islands themselves, in which event the border formations of the continent will present the same evidences of oceanward derivation of material that some of the coast formations of the continents do today. It is obvious, however, that, in this case, the phenomena will not imply a lost continent or any radical change in the configuration of the lithosphere, or even of the continent; much less will it afford evidence adverse to the essential permanence of the continents.

The intent of these suggestions is not to enforce any particular interpretation of the extremely valuable and suggestive data afforded by the oceanic islands, nor indeed to imply the limitation of alternative hypotheses to those here mentioned, but rather to emphasize the fact that the hypotheses employed in these inquiries, as in most others of a far-reaching nature, need to be traced back scrupulously to their sources, and to be put into comparison with other hypotheses, so that the true values and the limitations of the criteria employed may be made apparent, and their dependence upon fundamental hypotheses may be brought forth into sharp definiton and working application.

T. C. C.

The article in this number of the *Journal* on the Tertiary glaciation of Iceland possesses much interest for students of glacial geology in this country; but it should perhaps be pointed out that the Tertiary age of these morainic formations is not to be accepted, except on the basis of the most irrefutable evidence, and evidence of this sort is not cited in the article. The glacial deposits are interbedded with lava, and since the lava has been thought to be of Tertiary age, the moraines are inferred to be of Tertiary age. If the first of these conclusions is correct, the second is; but, so far as the evidence cited in this paper is concerned, the argument would seem quite as plausible if stated the other way, namely: Since the sediments interbedded with the lavas are glacial, they are Pleistocene; the lavas interbedded with them are therefore Pleistocene. Indeed, the presumption is strongly in favor of this statement, since abundant lavas of Pleistocene age are known, and no glacial formations of Tertiary age in any part of the earth are known, unless these constitute the exception. If they are really of Tertiary age, the fact is most significant, since it will call for a revision of present opinion concerning the climate of some part of the Tertiary period.

---

[1] *Op. cit.*, Vol. III, pp. 518–30.

On the other hand, great deformations, great erosion-unconformities, and great physiographic features due to erosion have been developed since the close of the Tertiary, and igneous eruptions of consequence are known to have taken place, in America at least, since the Tertiary period. Accumulating evidence makes it clear that the rather common conception that the Pleistocene period was a very short one, must be abandoned. The evidence is now altogether adequate to show that the period was long enough for changes and events far greater than those which have taken place in Iceland since the inter-lava drift of that island was deposited.

R. D. S.